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# Dilemmas in the Constitution of and Exportation of Ethological Facts

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# Dilemmas in the Constitution of and Exportation of Ethological Facts[1]

*Richard W. Burkhardt, Jr.*

**Abstract**

Early ethologists such as Niko Tinbergen and Konrad Lorenz faced a problem: What constituted a fact about behaviour? How reliably must a behaviour be exhibited (and in how many specimens) before it could be said to be species-typical? And how similar do the behaviours of two species need to be before it is reasonable to say that the behaviour is true of both? They sought to convince others of their claims for interspecific behavioural commonalities through a number of means – writings, diagrams, films – and enjoyed some notable successes. But establishing facts about behaviour that would hold across multiple species was a dispute still largely contained within the relatively esoteric discipline of ethology. It was only a matter of time before the species boundaries being crossed were more controversial. For if the problem of establishing that a fact about goose-behaviour is also a fact about duck-behaviour was of limited interest, it was of considerably more significance when one of those species was human. With the publication of works such as Lonrenz's *On Aggression* and E. O. Wilson's *Sociobiology*, what had been a marginal issue for zoology was now of considerable political significance, and the original claims for inter-specific behavioural similarities fell under renewed and intense scrutiny – leading to the reexamination of the original facts on which ethology was predicated.

In 1949, the fledgling ethologist Robert Hinde observed a happy interchange between Konrad Lorenz and Niko Tinbergen, the founders of ethology, in their first days together after the Second World War. The location was Cambridge, England. The occasion for the ethologists being in Cambridge was a special symposium on "Physiological Mechanisms in

[1] Sections of this paper are based on my book, *Patterns of Behavior: Konrad Lorenz, Niko Tinbergen, and the Founding of Ethology* (Chicago: University of Chicago Press, 2005), the research for which was supported by funding from the National Science Foundation (SOC78-05922 and SBE9122970), the John Simon Guggenheim Foundation (1992-1993), and the Research Board of the University of Illinois at Urbana-Champaign.

Animal Behaviour," hosted by the Society for Experimental Biology. The interchange in question happened outside of the official proceedings. As Hinde recalled:

> We were walking down Jesus Lane in Cambridge, and Tinbergen and Lorenz were discussing how often you had to see an animal do something before you could say that the species did it. Konrad said he had never made such a claim unless he had seen the behaviour at least five times. Niko laughed and clapped him on the back and said "Don't be silly, Konrad, you know you have often said it when you have only seen it once!" Konrad laughed even louder, acknowledging the point and enjoying the joke at his own expense.[2]

This story is instructive for what it tells about Lorenz and Tinbergen and their relationship to one another. It is also helpful in introducing the subject of the construction of ethological facts. Before addressing the topic of ethological facts *traveling*, however, it is worth saying something about the kinds of facts in which the ethologists were interested in the first place.

Central to the ethologists' enterprise was their identification of what they understood to be innate, *species-specific* behavior patterns. Innate behavior patterns, as Lorenz explained at the Cambridge meeting, are "something which animals of a species 'have got,' exactly in the same manner as they 'have got' claws or teeth of a definite morphological structure."[3] To Lorenz, the implications of this were far-reaching. The founding insight of his field – its "Archimedean point," as he liked to call it – was the notion that innate behavior patterns -- just like claws, teeth, or other body parts -- needed to be understood from "the comparative, evolutionary viewpoint." Instinctive behavior, in other words, could be used just like physical structures not only in identifying species but also in reconstructing phylogenies and assessing genetic affinities. For Lorenz,

[2] R. A. Hinde, "Nikolaas Tinbergen," *Biographical Memoirs of Fellows of the Royal Society* 36 (1990): 547–565, quote on p. 553.
[3] Lorenz, "The comparative method in studying innate behaviour patterns," *Symposia of the Society for Experimental Biology*, 4, (1950), p. 238.

this was the defining feature of his whole enterprise. Indeed, instead of the word “ethology,” he preferred to call his field “comparative behavior study” (*Vergleichende Verhaltensforschung).*

That said, we also need to consider how the ethologists positioned themselves with respect to other disciplines. Prior to the war, the ethologists were especially concerned with distinguishing themselves from animal psychologists. They had insisted that they were addressing critical biological questions that the animal psychologists were ignoring, most notably the questions of evolutionary history and survival value. In addition, they claimed that ethology represented a more *objectivistic* approach to behavior than did the approaches of such major, *subjectivistic* animal psychologists as the Dutch scientist J. A. Bierens de Haan. In 1942, in an early, programmatic statement of what ethology was all about, Tinbergen maintained that ethology’s aim was to understand innate behavior in physiological terms.[4] In Cambridge, England, seven years afterwards, the ethologists were in effect hoping to demonstrate how far they had come in this regard.[5] The conference had been organized by Tinbergen and W. H. Thorpe, the Cambridge entomologist-turned-ethologist. They wanted to set up a venue where ethologists could present the results of their research to physiologists. Later, in the 1950s, the ethologists’ primary target would be the American behaviorists.

The historical point to be stressed here is that with respect to facts described, questions asked, methods employed, and theories ventured, ethologists looked toward a number of different disciplines, at different times, with an eye to impressing or influencing practitioners in those areas. Over time, they broadcast the nature of their work through interdisciplinary seminars and conferences, public lectures, articles, books, films, and so

[4] N. Tinbergen, “An objectivistic study of the innate behaviour of animals,” *Bibliotheca Biotheoretica*, 1 (1942), 39-98.
[5] The conference is discussed in Burkhardt, *Patterns of Behavior*, pp. 306-325.

on. A number of scientists in other disciplines facilitated their efforts, while still other scientists criticized or ignored them. Among the examples of ethological facts traveling to be mentioned here, some were boosted in their travels by the images associated with them, others were aided by the activity of individuals friendly to the ethologists' cause, and others failed to reach their intended destination when the particular package in which they had been embedded was rejected as unwanted.

To be sure, the ethologists were not interested in transmitting just facts. The Cambridge conference of 1949 was where Lorenz presented his famous psycho-hydraulic model of instinctive action. There too Tinbergen presented a model of his own, that of the hierarchical organization of behavior. All the while, however, the ethologists took pains to stress the factual foundations of their models. Lorenz acknowledged the "extreme crudeness and simplicity" of his psycho-hydraulic model but insisted that the model symbolized, in his words, "a surprising wealth of facts really encountered in the reactions of animals."[6] In addition, he emphasized the strong, empirical inclinations of ethology's forefathers. Identifying the American biologist Charles Otis Whitman and the German ornithologist Oscar Heinroth as the two great pioneers of comparative ethology, Lorenz allowed that their achievements were due primarily to the fact that they were animal lovers and empiricists. Whitman's passion was pigeons; Heinroth loved ducks and geese. As Lorenz cheerfully described their work,

> Happily ignorant of the great battle waged by vitalists and mechanists on the field of animal behaviour, happily free from even a working hypothesis, two "simple zoologists" were just observing the pigeons and ducks they loved, and thus kept to the only way which leads to the accumulation of a sound, unbiased basis of induction, without which no natural science can arise.[7]

[6] Lorenz, "The comparative method in studying innate behaviour patterns," p. 255.
[7] Lorenz, "The comparative method in studying innate behaviour patterns," p. 222.

Probably everyone in Lorenz's audience recognized this as hyperbole. If not, they should have. Whitman was indeed a lover of pigeons, but he was also thoroughly engaged with the broadest questions of biology. Issues of evolution, heredity, and development constituted the *raison d'être* of Whitman's pigeon studies. The portrait of a happy empiricist does not suit him in the least. Heinroth, on the other hand, fits the picture better. He and his wife Magdalena, in their classic study on the birds of central Europe, operated on the assumption that what was innate and what was learned in different bird species could only be determined by means of experiments conducted on a species-by-species basis. Their painstaking multi-year project involved rearing individuals of every different central European bird species by hand, from the egg, and watching how each bird behaved from the time it hatched all the way to its adulthood.[8] Even Heinroth, though, was capable of looking up from his facts to see a broader vision. In 1910 he expressed what might be called the "sooner or later" motif of animal behavior studies, that is to say, the belief that such studies would ultimately have something of value to offer for understanding human behavior. At the international ornithological congress of 1910 he closed his paper on the ethology of ducks and geese with the prediction: "The study of the ethology of the higher animals—unfortunately a still very untilled field—will bring us ever closer to the realization that in our conduct with family and strangers, in courtship and the like, it is more a matter of purely inborn, more primitive processes than we commonly believe."[9]

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[8] Heinroth, Oskar and Magdalena Heinroth, *Die Vögel Mitteleuropas in allen Lebens- und Entwicklungsstufen photographisch aufgenommen und in ihrem Seelenleben bei der Aufzucht vom Ei ab beobachtet*, 4 vols. (Berlin: H. Bermühler, 1924-1934).

[9] Oskar Heinroth, Beiträge zur Biologie: namentlich Ethologie und Psychologie der Anatiden," in *Verhandlungen des 5. Internationalen Ornithologen-Kongresses in Berlin, 30 Mai bis 4. Juni 1910*, ed. Herman Schalow, pp. 589-702 (Berlin: Deutsche Ornithologische Gesellschaft), p. 702. All translations from the German are by the author.

Lorenz would embrace this goal a generation later. In 1931, not long after becoming acquainted with Heinroth and Heinroth's work, Lorenz wrote ecstatically to the older man saying: "Who knows what will become of today's human psychology if one can only know what is instinctive behavior and what is rational behavior in humans? Who knows how human morals with their drives and inhibitions would look if one could analyze them like the social drives and inhibitions of a jackdaw."[10] From the 1930s onward, Lorenz was keen to proclaim that the study of animal social instincts would shed light on human social instincts. This appears indeed to have been one of the reasons he welcomed the German takeover of Austria in the spring of 1938. In the years immediately preceding that, he had begun to believe that his career as a scientist in Austria was being thwarted by the Catholic educational establishment, which wanted no part of his ideas about the animal roots of human behavior. He imagined that the Third Reich would provide a more receptive *Weltanschauung* for his ideas.[11]

We will come back to the topic of extrapolating from animal behavior to human behavior. For now, let us shift attention to the relations between early ethology and American comparative psychology, with special attention to facts crossing borders.

In 1899, Charles Otis Whitman threw a gauntlet down to modern animal psychologists. In a paper entitled "Myths in animal psychology," he skewered a handful of writers who had misinterpreted various facts of animal behavior. One of the authors was the Englishman George John Romanes. Romanes had repeated an account provided to him by an English lady, who had described the way a male pigeon performed courtship displays to a ginger beer bottle whenever the bottle was put in

[10] Heinroth, Oskar and Konrad Lorenz, *Wozu aber hat das Vieh diesen Schnabel? Briefe aus der frühen Verhaltensforschung, 1930-1940*, edited by Otto Koenig (Munich: Piper, 1988), p. 42.
[11] See Burkhardt, *Patterns of Behavior*, chapter 5.

the bird's vicinity. Romanes offered the bird's behavior as an instance of avian insanity. Whitman, who knew pigeon behavior better than anyone else, offered Romanes's analysis instead as an example of how far one could go astray if one had not first gained a thorough knowledge of the normal behavior of the species in question. In Whitman's words: "The qualification absolutely indispensable to reliable diagnosis of an animal's conduct is an intimate acquaintance with the creature's normal life, its habits and instincts. Little can be expected in this most important field of comparative psychology until investigators realize that such qualification is not furnished by parlor psychology." What was required, he continued, was nothing less than years of close study…." Later in his paper Whitman complained again of "students ambitious to reach the heights of comparative psychology through a few hours of parlor diversion with caged animals, or by a few experiments on domestic animals."[12]

These themes would be repeated half a century later. Ethologists would insist that the first thing a student of animal behavior needed to do was to learn the full behavioral repertoires of the particular species in which he or she was interested. Ethologists would furthermore complain about the psychologists' use of a limited number of highly domesticated animal races, especially the white rat. By the 1940s and 1950s, however, they could no longer claim that the psychologists had spent just a few hours in their studies. American comparative psychology had by this time put hundreds of researchers to work for their entire careers doing experiments on learning in the white rat.

We will return to the comparative psychologists, but first let us consider an interesting experiment conducted by Tinbergen and Lorenz in the spring of 1937 when Tinbergen spent three months at Lorenz's home in Altenberg, Austria. The two zoologists never wrote up the experiment

[12] C. O. Whitman, "Myths in animal psychology," *Monist* 9 (1899), 524-537.

fully, but Lorenz described it briefly in a paper of 1939, and Tinbergen did the same, with illustrations, in a paper of 1948 and then again in his book, *The Study of Instinct*, in 1951.[13] The experiment tested the reactions of hand-reared fowl of various species to simulated flying predators, the latter being dummies of a variety of shapes made from cardboard. The experimenters strung up a rope between two tall trees and pulled the dummies along the rope to mimic the motion of birds in flight.

Tinbergen and Lorenz tried their experiment on virtually all the young fowl that Lorenz had at Altenberg in the spring of 1937. Young greylag geese, turkeys, and numerous species of ducks were all tested. The cardboard dummies were pulled along the rope, above the birds, at different speeds and in both directions. The results, significantly enough, differed per species. It was not the case for the geese and the ducks, but for young turkeys the *shape* of the moving dummy seemed to make a difference. Dummies with "short necks" elicited the turkeys' alarm calls much more readily than did dummies with "long necks." Most remarkably, the investigators found they could actually evoke these results with a single, relatively crude dummy constructed with the "wings" located toward one end of the body in such a way as to make one end of the body short and the other long. Which end appeared as the "head" and which appeared as the "tail" depended on the direction in which the dummy was pulled. The young turkeys displayed the most alarm when the dummy was moved slowly above them with its short end forward and its long end to the rear. When the dummy was moved with its long end forward, the turkeys were calmer.

[13] See Konrad Lorenz, "Vergleichende Verhaltensforschung," *Verhandlungen der Deutschen Zoologischen Gesellschaft, Zoologischer Anzeiger,* supp. 12 (1939): 69–102, on pp. 92–94; N. Tinbergen, "Social releasers and the experimental method required for their study," *Wilson Bulletin* 60 (1948): 6–51, on p. 7; N. Tinbergen, *The Study of Instinct* (Oxford: Oxford University Press, 1951), on pp. 77-78. Tinbergen's field notes of the experiments, dated from 16 March to 11 June 1937, are among the Nikolaas Tinbergen papers preserved at Oxford University at the Bodleian Library, Department of Special Collections and Western Manuscripts.

**Figure 1:** Tinbergen's illustration of some of the different shapes he and Lorenz used for dummies when testing the innate fear responses of juvenile birds. From Tinbergen (1948).

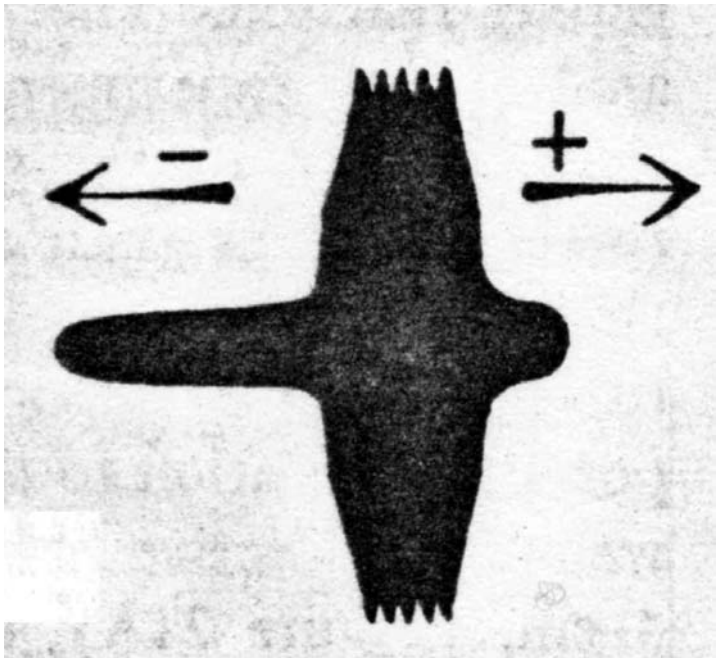


**Figure 2:** Tinbergen's illustration of "a card board dummy that elicits escape reactions [in young turkeys] when sailed to the right ('hawk') but is ineffective when sailed to the left ('goose'). From Tinbergen (1948).

The two naturalists concluded that the difference corresponded to the basic shapes of avian predators versus avian nonpredators. Predators like hawks have short necks relative to the rest of their bodies. Nonpredators like geese have long necks relative to the rest of their bodies. Tinbergen and Lorenz concluded that the young turkeys' response to the gestalt of the slowly moving, short-end-forward shape was an innate

response, forged by natural selection, to an environmental cue signaling "predator."

Here, certainly, was an experiment involving specific facts. Here too was an explanation to go with them. These traveled far enough beyond the bounds of ethology to elicit a challenge in 1955 from three American psychologists: Jerry Hirsch, R. H. Lindley, and E. C. Tolman. They undertook to replicate the Tinbergen-Lorenz experiment using white leghorn chickens. Failing to get similar results, they summarized their findings as follows: "The Tinbergen hypothesis that certain specifically shaped sign stimuli innately arouse a fear response was tested on the white Leghorn chicken and found to be untenable under controlled laboratory conditions."[14] This conclusion drew critical responses from both Tinbergen and Lorenz. As Tinbergen put it "Whatever the shortcomings of 'ethological' studies may be, one thing they have demonstrated convincingly: the fact that different species usually behave differently in the same situation." The obvious implications of this were that "Facts found in one species, or hypotheses formed about one species, simply cannot be disproved by testing another species, under however well 'controlled laboratory conditions.'" He additionally observed that the white leghorn chicken was a poor choice as a test animal, stating: "it is known that the behavior of domesticated forms often differs considerably from that of the wild ancestral forms." Lorenz echoed Tinbergen's criticism of the American psychologists' paper. It was as if, Lorenz said, one scientist reported finding melanins in the fur of wild hamsters and another scientist claimed

[14] Jerry Hirsch, R. H. Lindley, and E. C. Tolman, "An experimental test of an alleged innate sign stimulus," *Journal of Comparative and Physiological Psychology*, 48 (1955), 278-280, quote on p. 280.

to refute this by saying that his own studies on white, laboratory rats showed the hamster results to be untenable.[15]

The ethologists' response was entirely valid. It does not negate it to note that in 1961, Wolfgang Schleidt, one of Lorenz's students, redid the Tinbergen-Lorenz turkey experiment and found that young turkeys do not respond to specific shapes but rather to the speed at which the shapes moved. [16] Nor is the ethologists' response negated by the fact that Lorenz himself often leapt from one species to another in ways that left other scientists uncomfortable.

But we are still not done with the hawk-goose story, which has yet more to offer with respect to the theme of facts traveling. (The author thanks Wolfgang Schleidt for the details that are to follow.) Despite the doubts that Schleidt's turkey experiments cast on the interpretation of the original experiments of 1937, the images from Tinbergen's 1948 paper and from his 1951 book, *The Study of Instinct*, continued to be reproduced. They appeared on the covers of two important animal behavior textbooks of the 1960s, Peter Marler and William J. Hamilton III's *Mechanisms of Animal Behavior* (1966), and Aubrey Manning's *An Introduction to Animal Behavior* (1967).[17] The cover of the Marler and Hamilton book featured the various shapes of the dummies that Tinbergen had used in the experiments. Manning's book cover used the single, two-directional, "hawk-dove" model.

Manning was familiar with Schleidt's study, and he cited it. Manning's summary conclusion, nonetheless, was that "there is evidence

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[15] N. Tinbergen, "On anti-predator responses in certain birds – a reply," *Journal of comparative and physiological psychology,"* 50 (1957), 412-414, quotes from pp. 412-413. Konrad Lorenz, *Evolution and Modification of Behavior* (Chicago: University of Chicago Press, 1965), p. 100.
[16] W. M. Schleidt, "Reaktionen von Truthühnern auf fliegende Raubvögel und Versuche zur Analyse ihres AAM's," *Zeitschrift für Tierpsychologie* 18 (1961): 534–560.
[17] Peter Marler and William J. Hamilton, III, *Mechanisms of Animal Behavior* (New York: Wiley, 1966); Aubrey Manning, *An Introduction to Animal Behavior* (Reading, Massachusetts: Addison-Wesley,1967).

that wild birds do possess an IRM [Innate Releasing Mechanism] which enables them to respond to birds of prey on the first occasion that they see them. This IRM probably has different properties in different species but short neck and relative speed of movement are among them."[18] Less nuanced was the response of the distinguished ornithologist and secretary of the Smithsonian Institution, S. Dillon Ripley, an admirer of the work of Lorenz & Tinbergen. Ripley became a major proponent of the idea that the silhouette of a raptor could deter songbirds from flying into windows. Under his direction the Smithsonian Museum shop began marketing raptor stickers to put on windowpanes. Such stickers continue to be used today, without any evidence that they actually work.[19]

Let us return now to the fact of significant interspecific differences in behavior. It would be worth attempting to track how this fact made its way into American comparative psychology. That this appreciation was needed was signaled not only by the Continental ethologists but by a few American comparative psychologists as well. Prominent among them was Frank Beach. In his article of 1950 entitled "The Snark was a Boojum," Beach indicted American comparative psychology as not having been genuinely comparative for years. He demonstrated this through an analysis of the papers published in the field's primary journals over the previous four decades. In addition to displaying his findings graphically, he characterized the predicament with a cartoon, inspired by the story of the Pied Piper of Hamelin. In the cartoon, the familiar roles of humans and rodents were reversed. A rat (a white one) played the tune, while the people, a crowd of scientists, followed eagerly behind, unaware they were being led to their doom.[20]

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[18] Manning, *An Introduction to Animal Behavior*, p. 53.
[19] Schleidt, personal communication with the author.
[20] Beach emphasized his point further by noting that his field's major journal perhaps ought to be called "The Journal of Rat Learning." The trouble with doing that, he said, was that many

When Beach went on to discuss the potential benefits of a genuinely comparative approach, the first two authors he cited, even though he was talking about learning rather than instinct, were Tinbergen and Lorenz. He cited Tinbergen for his studies on learning in the hunting wasp. He cited Lorenz for his observations on imprinting in precocial birds.[21]

Beach's case would be worth a more extended examination than can be provided here. Lorenz, in a letter to W. H. Thorpe in 1955, described how he had made a convert out of Beach by showing him films. In Lorenz's words: "The best means to convince people that there is such a thing as instinctive movements is the film. I played duck films to Frank Beach until he nearly fainted, he got seriouser and seriouser and in the end he said in a small voice: 'You know I did not believe a word of it and now I believe everything.'"[22] Judging from a paper Beach published the very same year, however, it is hard to countenance the idea that he now believed *everything* that Lorenz wanted him to believe about instinct. In his 1955 paper, entitled "The Descent of Instinct," Beach suggested that when the development of behavior in the individual came to be properly analyzed, the concept of "instinct" would not be needed.[23] Evidently while Beach felt there were things that American comparative psychologists could learn from continental ethology, he likewise thought there were

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psychologists would not see the purpose of doing so, given that they already supposed that "in studying the rat they [were] studying all or nearly all that is important in behavior." Beach noted how the leader of the discipline, B. F. Skinner, had entitled his book *The Behavior of Organisms*, even though it was based, in Beach's words, "exclusively upon the performance of rats in bar-pressing situations." Frank A. Beach, "The Snark was a Boojum," *American Psychologist*, 5 (1950), 115-124, quote on p. 119.

[21] Beach's 1950 paper was preceded by a 1946 paper by his American comparative psychologist colleague, T. C. Schneirla, who cited Tinbergen to the effect that American comparative psychology was not really comparative. See Schneirla, "Contemporary American animal psychology in perspective," in *Twentieth Century Psychology*, ed. P. L. Harriman (New York: Philosophical Library, 1946), pp. 306-316.

[22] Beach was enlisted by Tinbergen as the first American on the editorial board of the new ethological journal, Behaviour. Later, Beach was a regular participant in the International Ethological Congresses and a member of the organizing board for these conferences. For Lorenz's letter to Thorpe, see Burkhardt, *Patterns of Behavior*.

[23] Frank A. Beach, "The Descent of Instinct," *Psychological Review*, 62 (1955), 401-410.

insights that ethologists could gain from American comparative psychology.

The present story would be simple enough if the ethologists' fact of significant interspecific differences in behavior made steady inroads into American comparative psychology after 1950. It did make inroads, but only haltingly, and with qualifications. There were several reasons for this. In the first place, American comparative psychologists as a group were simply not interested in details about animal behavior that occurred in naturalistic rather than laboratory settings. In the second place, it was one thing to acknowledge the existence of species-specific differences in behavior but quite another to accept the ethologists' assumption that the behavior in question deserved to be called innate. In the third place, in acknowledging the importance of interspecific behavioral differences, the comparative psychologists could turn the tables on the ethologists and criticize them for having too readily supposed that behavior displayed at one level of organic complexity was comparable to behavior displayed at another level of complexity. These last two points were among those presented by the American psychologist Daniel Lehrman in his famous critique of Lorenzian ethology in the *Quarterly Review of Biology* in 1953. Lehrman's paper also took note of the political dimensions of Lorenz's writings. Criticizing Lorenz for "[equating] the effects of civilization in human beings with the effects of domestication in animals," Lehrman observed that Lorenz had promoted this idea in 1940 in support of German race purity laws.[24]

Associations such as these are not incidental to how facts travel. The question of Lorenz's wartime affiliations would continue to lurk in the background as Lorenz offered new pronouncements about the biological

[24] Daniel S. "A Critique of Konrad Lorenz;s theory of instinctive behavior," *Quarterly Review of Biology*, 298 (1953). 354.

bases of human behavior and how these related to the human predicament. Of these new pronouncements, two in particular were especially eye-catching. Lorenz's first claim was that the human species is unique among higher animals in that it lacks innate inhibitions against killing its own kind. The second claim was that aggression is an instinct, and that, as such, it builds up internally, like a fluid in a reservoir, eventually requiring release. Lorenz presented both of these claims essentially as facts, though neither is credited with that status today, nor were they universally regarded as such when Lorenz first pronounced them.

Lorenz highlighted the first of these claims with a striking contrast between wolves and doves. Wolves, he allowed, have been equipped by evolution not only with fearsome weapons—their strong jaws and their sharp teeth —but also powerful, instinctive inhibitions against using these weapons against other wolves. When two wolves fight, and one gets the better of the other, Lorenz explained, if the loser submissively exposes its neck to its adversary, the victor cannot finish the loser off. Instinctive inhibitions prevent it from doing so.

Doves, in comparison, have no powerful natural weapons. Because of this, they have not had to develop inhibitions against hurting their own kind. In nature, by Lorenz's account, when two doves fight, the bird that loses can simply fly away. If the birds are confined to a cage, however, fleeing is impossible, and the weaker bird is in danger of being killed, because the winner has no innate inhibitions against continuing the fight to the end. Lorenz described how he placed a male turtledove and a female African blond ringdove together in the same cage, hoping they would mate. When he returned, he found that the ringdove had nearly pecked the turtledove to death.

Were there implications here for the human species? Lorenz believed there were. The human species, he argued, is more like the dove than the wolf when it comes to dealing with its own kind. Humans do not have powerful natural weapons, like wolves do, and thus until relatively recently, evolutionarily speaking, humans have had no need to develop strong instinctive inhibitions against killing one another. Unfortunately, in the latest stages of our history, we humans have had our science and technology far outpace our biological evolution. That is to say, we have developed artificial weapons of tremendous destructive power without developing instinctive inhibitions against using them.

There are multiple problems with the story that Lorenz constructed. One is that in citing the example of a ringdove nearly pecking to death a bird of another species, a turtledove, he was arguing past the question of whether animals of the same species kill each other. A second is that he was completely ignoring the testimony of the leading expert on dove behavior in the first half of the century, Wallace Craig (whose work Lorenz in fact knew). Craig had already insisted that doves of the same species do not go on fighting each other in the way that Lorenz went on to claim that they did.[25] A third problem, stemming from evidence on animal behavior collected in the years since Lorenz made his claim, is that among non-human higher animals the killing of members of one's own species occurs in some species not simply as an occasional accident, as Lorenz maintained, but more systematically, as in the case of male lions killing off the cubs of other sires, or male chimpanzees killing the infant chimps or other members of another chimp tribe. This last point is a finding that would have made no sense to Lorenz, given his predilection for "good of the species" type arguments. It becomes more understandable in the context of the kind "selfish gene" thinking that developed in the 1970s.

[25] I discuss this more extensively in Burkhardt, *Patterns of Behavior*, pp. 451-453.

The present writer has not attempted to track how far this first claim by Lorenz traveled.[26] It seems, in any case, that Lorenz's ideas on aggression traveled farther, at least in the sense that they were more widely and recurrently debated. What Lorenz claimed to offer in his best-selling book of the 1960s, *On Aggression*, was an analysis of the natural history of aggression. His basic message was that the human race had to come to understand its instinctive aggressive drives in order to learn how to deal with them, and that essential to this understanding was a recognition of the positive as well as the negative aspects of aggression. Although man was faced with a predicament of the most urgent sort —"in his hand the atom bomb, the product of his intelligence, in his heart the aggression drive inherited from his anthropoid ancestors"—Lorenz was prepared to offer an "avowal of optimism." He believed the biologist could rescue humankind from its precarious state by teaching humans to change for the better.[27]

Lorenz's portrayal of aggression attracted a great amount of attention. The critics included ethologists as well as representatives of other disciplines. They rejected in particular his claim that aggression is an instinct that builds up internally and requires release.[28] Even as an idea that has been rejected, this one still has a certain staying power. Introductory psychology textbooks still cite Lorenz with some frequency. He is the only one of the early ethologists, it seems, for which this is true.

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[26] Lorenz's claim was repeated elsewhere. Anthony Storr, in his 1968 book, *Human Aggression*, asserted that aside from rodents, man is the only vertebrate who "habitually destroys member of its own species." For an ethologist's assessment of animal aggression as of 1976, see Peter Marler, "On animal aggression: the roles of strangeness and familiarity," *American Psychologist*, 31 (1976), 239-246.

[27] Lorenz, *On Aggression*, pp. 49, 275.

[28] One of the more gentle critics was Niko Tinbergen. Tinbergen's position was that what ethology had to offer in the way of understanding of human behavior was not facts about other species so much as ethology's whole approach of looking carefully at a species and considering the causation, development, evolution, and survival value of the species' behavior. What Lorenz had offered and what Tinbergen's student Desmond Morris had offered in his book, *The Naked Ape*, were in Tinbergen's view "no more than likely guesses."

He is remembered in two contexts: One as the author of an interesting but generally discredited theory of aggression; the other as the scientist who called attention to the phenomenon of imprinting.

Most of the "traveling facts" offered thus far in this paper have straddled the line between facts and theories. Let us consider yet another case, in this instance one where where some of Tinbergen's facts passed beyond ethology to the realm of American social science education. This occurred in the federally-funded social science curriculum entitled "Man: A Course of Study" – or MACOS. The prime mover of the curriculum was Jerome Bruner, the cognitive psychologist who was co-founder and Director as of 1960 of the Center for Cognitive Studies at Harvard. Bruner's desire was to "form the intellectual powers" of the students the curriculum was supposed to serve, namely elementary school students in the fifth and sixth grades. He wanted students to become self-conscious about their strategies of thought. The content of MACOS was identified in 1965 as "man: his nature as a species [and] the forces that shaped and continue to shape his humanity." The three recurring questions of the course were to be: (1) "What is human about human beings?" (2) "How did they get that way?" (3) "How can they be made more so?"[29]

Early on, the developers of MACOS planned to use only one animal species, the savannah baboon, as a contrast with humans. The trouble with this approach, as it turned out, was that the elementary school pupils saw baboons as being so similar to humans they had trouble identifying strong differences between the two.[30] To underscore certain differences more carefully, the educators introduced two more species: the Pacific

[29] J. S. Bruner, *Man: A Course of Study*. Occasional Paper No. 3, The Social Studies Curriculum Program, Educational Services Inc. (Cambridge, MA, 1965), p. 4. Cited in Peter B. Dow, "Man: A Course of Study: A continuing exploration of man's humanness," in *Man: A Course of Study. Talks to Teachers. 1983 Edition* (Curriculum Development Associates: Washington D.C., 1983), p. 4.

[30] Curriculum Development Associates, *Man: A Course of Study. A Guide to the Course 1976 Edition* (Curriculum Development Associates: Washington D.C., 1976), p. 26.

coast salmon and the herring gull. Baby salmon must do without parental protection in their struggle to survive. Their story was used to highlight the significance in humans of the length and the quality of the *human* infant's dependence on its parents. Herring gull chicks, unlike baby salmon, are taken care of by their parents. The gull story, based on the work of Tinbergen, was used to examine more closely the *causes* of animal behavior. Observations of how the gull chicks must peck at the red spot on their parent's beak if they are to be fed provided an entry to the discussion of innate versus learned behavior. The herring gull section also helped introduce the idea that behavior patterns, like physical structures, should be understood in terms of their adaptiveness or survival value.[31] Beyond this, the herring gull study was intended to give children the opportunity to study territoriality, fighting, and communication. The authors of MACOS suggested that children are intrigued by the idea of an aggressive instinct, and the gull study would allow them to "consider the ways a human handles his aggressive feelings without really fighting." They recommended that children be given a chance to act out scenes of adult male fighting in herring gulls, where the use of particular bodily gestures enables the antagonists to escape serious harm. They also suggested that the teachers go to Lorenz's book, *On Aggression*, for helpful background reading.[32]

The teachers were introduced to the concept of natural selection by a short piece written by the evolutionary theorist Robert Trivers. Trivers' concluding observation was that one could not legitimately talk about higher versus lower animals, or more evolved versus less evolved animals.

[31] Teachers were encouraged to read Tinbergen's *The Herring Gull's World* and chapter six of his *Animal Behavior*.

[32] In Teacher's Guide Number 4, *Herring Gulls*, pp. 21-22. The curriculum developers also recommended as an "optional reading assignment": "You or one of the better readers in your class might read to the children parts of the last chapter of Lorenz's *King Solomon's Ring*. This chapter describes many instances of animals fighting each other, and Lorenz discusses the gestures they use to keep from inflicting serious harm upon each other." P. 24.

As he put it, “in different environments, different characteristic are adaptive.” Expressing a theme that would recur at different levels through the course, Trivers wrote: “There are no traits in this scheme that have an absolute value, an absolute value irrespective of the environment.”[33]

The notion of no traits having an absolute value irrespective of the environment was what ultimately caused trouble for the MACOS curriculum. Perhaps no one would have objected if the story had stopped with herring gulls or even baboons, but when it was applied to human behavior, as exemplified by the lives of Netsilik Eskimos, this was too much for people who believed that human values are God-given.

In her book, *Science Textbook Controversies and the Politics of Equal Time*, Dorothy Nelkin describes what transpired. United States Congressman John Conlan of Arizona in 1974 described MACOS as “a Godawful course,” “almost always at variance with the beliefs and values of parents and local communities.” He urged that National Science Foundation appropriations for MACOS be terminated because of its “abhorrent, repugnant, vulgar and morally sick content.” Federal funds were withdrawn, and textbook sales dropped sharply between 1974 and 1975.[34]

While MACOS stirred up one angry group, E. O. Wilson’s book, *Sociobiology: the New Synthesis*, published in 1975, stirred up another. The most vocal protesters in the second case were not conservative, fundamentalist Christians but instead the radical scientists who constituted themselves as the Sociobiology Study Group of Science for the People. In

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[33] Robert Trivers, “Natural Selection,” in *Man: A Course of Study. Talks to Teachers* (Curriculum Development Associates: Washington D.C., 1970 [1983 edition]), pp. 35-41, quotation on p. 41. This volume, *Talks to Teachers*, also included a section by Tinbergen entitled “The Study of Animals,” extracted from his book *Animal Behavior* (New York: Time-Life Books, 1965), and a section by Irven Devore, with the assistance of R. Trivers and I. Rothman, entitled “Innate and Learned Behavior.”

[34] Dorothy Nelkin, *Science Textbook Controversies and the Politics of Equal Time* (Cambridge, Massachusetts: MIT Press, 1977), p. 112.

their attack on Wilson, they lumped him together with Lorenz as a biological determinist and noted that Lorenz had been associated with the Nazis. But there were other critics as well. One of these was the American psychologist Frank Beach. Prominent among Beach's complaints was "[the sociobiologists'] apparent omission or disregard of facts concerning interspecific similarities and especially interspecific differences." Explaining the flaws in Wilson's discussions of what Wilson blithely termed "homosexuality" in a wide range of animals, Beach wrote: "There is a fundamental rule that applies to all such cases whether whether the comparison is between animals and humans or between different species of animals. *The validity of interspecific comparison is limited by the reliability of intraspecific analysis*.

Meaningful comparisons between Species A and Species B simply are not possible until the behavior in question has been analyzed with equal care, objectivity, and precision *in both species*."[35]

Earlier in this paper, when discussing Heinroth's comments on animal and human social instincts, we spoke of the "sooner or later" motif of animal behavior study. A second key motif of animal behavior studies seems to have been the one we have just seen Beach expressing, namely, that in seeking implications for humans through the study of animal behavior, close scrutiny is critical in constituting the facts in the first place, for all the species concerned, before concluding how far these facts might appropriately travel. These motifs are mirror images of each other. The ongoing tension between them, closely tied to the question of which facts should or should not be allowed or encouraged to "travel," has much to do with the perennial fascination of studying animal behavior.

[35] Frank A. Beach, "Sociobiology and interspecific comparisons of behavior," in Michael S Gregory, Anita Silvers, and Diane Sutch, eds., *Sociobiology and Human Nature: An Interdisciplinary Critique and Defense* (San Francisco: Jossey-Bass, 1978), pp. 116-135, quote on p. 131.

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